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tawny-coloured surface of the equatoreal belt. The drawings I have made do not show any remarkably symmetrical forms; in several instances I had drawn nearly oval markings or turreted forms on the southern edge of the equatorial belt, but using the highest powers the night would bear, and watching for fitful intervals of the best definition, I always found these exceedingly regular forms were incorrect, and had to modify them: for this reason I rather distrust the drawing made on the 3rd of March. The markings on the dark belt, south of the equator, present forms similar to those we see at times in white cumulus clouds previous to a thunder-storm. I believe that could I have obtained clearer views of the planet, I should not have left these markings quite so regular as they now appear in the drawing. When favourably seen, such markings usually terminate in points or rugged edges towards the W. The light belts frequently incline at a considerable angle from the poles towards the equator of the planet. The belt shown on the drawing No. 4, made on May 5th, at 9.15 P.M., G. M. T., makes an angle of nearly 25° with the equatorial belt; a number of shorter markings, also, at about the same angle, stretch some distance into the tawny-coloured equatorial belt; these I at first drew of a turreted form, and only very close examination convinced me that they had the inclination I have now given to them. The darker belts have varied from dark warm or cool grey, to purple madder, or madder brown; but towards the poles of the planet the belts have been usually bluish grey, while close to the poles the blue colour has been very decided. During the month of May the colour of the equatorial belt had seemed fainter, but this is probably due to the planet being less favourably situated for observation.

I have made several observations of the spectrum of the planet, but though I have 12 inches of aperture, I do not find this sufficient to enable me to see more than the dark absorption bands in the red portion of the spectrum, with which most observers are now probably familiar. Though I cannot make out any differences in the appearance of the spectrum sufficiently marked to enable me to draw or describe them, yet I strongly suspect a change in the spectrum, which a larger aperture, giving more light, would enable me to bring out.

On the Masses of the Planets and the Parallax of the Sun.

By M. Le Verrier.*

The exact determination of the angle π, or the solar parallax is a matter of great interest to astronomers; it is the maximum angle under which an observer, supposed to be placed at the Sun's centre, would see the radius of the terrestrial globe.

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* Translated from the *Comptes Rendus* for 1872, July 22, by W. T. Lynn, B.A., F.R.A.S.

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The solar parallax being known in sexagesimal seconds, it is only necessary to find how many times it is contained in the number 206265, to conclude from it the Sun's distance from the Earth, referred to the radius of our globe as unity.

Laplace, in common with the other French astronomers of his epoch, adopted in the *Mécanique Céleste* the parallax 8″·813, derived from the transits of *Venus* observed in 1761 and 1769. From this it had been concluded that the Sun's distance from the Earth was 23405 radii of the Earth.

Afterwards Encke, having re-discussed the observations of those transits, determined that the parallax was only 8″·578, and that the Sun's distance ought consequently to be regarded as 24046 terrestrial radii.

It has been perceived that the change made by the Berlin astronomer in Laplace's number was not a happy one, and that, instead of taking anything away from it, the value 8″·813, attributed to the solar parallax in the *Mécanique Céleste*, should rather have been increased by some hundredths of a second.

Whether the Sun's distance from the Earth is greater or less by a small fraction of its amount, is not, considered in itself, a matter of such great interest. But the knowledge of the solar parallax is useful in many astronomical calculations: in particular, it gives the means of determining the value of the Earth's mass, and of taking account of its action upon the Moon, by introducing it into the calculations of the *Mécanique Céleste*.

Newton has furnished for this purpose a method which comes, in fact, following the *Mécanique Céleste*, to the employment of the formula

$$m = 4{\cdot}4320 \left(\frac{\pi}{1000}\right)^3,$$

m being the value of the Earth's mass referred to that of the Sun as unity.

If we admit 8″·813 as the value of π as the parallax, we find that the ratio of the mass of the Earth to that of the Sun is $\frac{1}{329630}$. The reduction of $\frac{1}{38}$th, proposed by Encke in the value of the parallax, would lead us to attribute to the Earth a mass less by $\frac{1}{13}$th than that which we have just given.

The preceding formula may also be solved with respect to π, which gives

$$\pi = 608{\cdot}79\, \sqrt[3]{m}.$$

This shows that if we could determine directly the value m of the Earth's mass, we could deduce the solar parallax therefrom.

If we set out by determining the parallax, the relative error that may be committed becomes threefold in the deduction of the value of the mass of the Earth.

If, on the other hand, we commence by determining the Earth's mass, the relative error in the deduction of the parallax becomes three times smaller. An error of $\frac{1}{300}$ in the value adopted for the Earth's mass corresponds to an error of $\frac{1}{900}$ in

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the value concluded for the parallax, that is to say, to very nearly 0″·01.

The attraction of the Earth produces sensible inequalities in the motions of *Venus* and *Mars*. By determining the amplitude of these by observations, it is possible to conclude directly from them the mass of our planet. It may be worth while to examine to what degree of exactitude we can arrive by this means.

To increase the precision of the result, we may unite the consideration of the periodic inequalities with that of the secular inequalities.

The elements of the orbits of *Venus* and *Mars* undergo variations called secular, which increase from year to year, from century to century, and at last acquire very large values, which renders them very suitable for resolving the question with which we are now occupied. It may even be said, *à priori*, that their consideration offers a method which ought, *in the course of time*, to equal in precision that which may be expected from the direct measure of the solar parallax, and at last even to exceed it. Let us inquire whether this epoch may not have now arrived.

At the time of the celebrated transits of *Venus* over the Sun in 1761 and 1769, Bradley had, ten years before, commenced the series of meridian observations which Maskelyne, Pond, and Airy have continued to the present time without interruption. By establishing at Greenwich the instrument invented fifty years before by Roemer, Bradley secured for England the possession of one of the fundamental bases of exact astronomy. This is what might have been done for France from the commencement of the eighteenth century.

The labours of Bradley gave the means of determining with accuracy the elements of the orbits of *Venus* and *Mars* for the epoch 1755. But nothing else of a precise nature was possessed in the way of observations, and it was necessary that long years should elapse before it was possible to demonstrate and measure the *variations* of the elements of the orbits. One hundred and twenty-one years have now passed away since Bradley's observations were made.

The variations of the excentricities, of the perihelia, of the inclinations, and of the nodes of *Venus* and *Mars*, may all be made to contribute to the result. Let us pause at the most important, that of the perihelion of *Mars*. The planet is in a position to be observed with great accuracy at its oppositions ; and, moreover, we possess a triple simultaneous observation made on the 1st October, 1672, by Picard, Roemer, and Richer, which extends to two hundred years the period of which we are able to avail ourselves.

The attraction of the Earth changes by 50″ in a century the heliocentric position of *Mars'* perihelion.

When *Mars* is actually in opposition, this change subtends at the Earth an angle of 185″.

The variation in two centuries and for the position of the

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planet *Mars* on the 1st of October, 1672, is, seen from the Earth, 294'' of longitude.

The discussion of the great series of meridional observations leads us to estimate that we can generally deduce from them, within a second, the value of the geocentric errors resulting from the consideration of the whole mass of observations. We may, therefore, consider that the epoch has arrived when the direct determination of the Earth's mass can be obtained, by the employment of the secular variations of the elements of the orbits of the planets, with a precision at least equal to that afforded by the direct observation of the Sun's parallax.

Let

$$(1)\quad \left\{\begin{array}{lll} m = 0{,}000000333\,(1 + \nu) & \ldots & \text{Mercury,} \\ m_1 = 0{,}000002489\,(1 + \nu_1) & \ldots & \text{Venus,} \\ m_2 = 0{,}000002817\,(1 + \nu_2) & \ldots & \text{The Earth,} \\ m_3 = 0{,}000000373\,(1 + \nu_3) & \ldots & \text{Mars,} \\ m_4 = 0{,}000952381\,(1 + \nu_4) & \ldots & \text{Jupiter,} \end{array}\right.$$

represent the real masses of *Mercury*, *Venus*, the *Earth*, *Mars*, and *Jupiter*; the numerical co-efficients representing the values of the masses which in the *Annales* have been used as starting-points, and ν, ν_1, ν_2, ν_3, ν_4, being the indeterminate quantities to be investigated by making them satisfy the observations.

The mass of *Mercury* results from the perturbations which that planet produces in the motions of *Venus*. The discussion of the observations made at Greenwich from 1751 to 1761, and from 1766 to 1830, furnishes as equation of condition:—

$$(2)\qquad 18''\cdot2\,\nu + 30''\cdot1\,\nu_1 + 35''\cdot35\,\nu_2 + 3''\cdot55 = 0.$$

Venus changes the position of the plane of the ecliptic. In order to satisfy the observed motion, we have the equation:—

$$(3)\qquad + 0''\cdot53\,\nu + 28''\cdot88\,\nu_1 + 0''\cdot83\,\nu_2 + 1''\cdot72 = 0.$$

The mass of *Mars* results from the discussion of the meridional observations of the Sun. This gives

$$(4)\qquad \nu_3 - 0''\cdot016\,\nu + 0''\cdot484\,\nu_1 + 0''\cdot071 = 0.$$

The observations of the fourth satellite of *Jupiter* and the perturbations of the system of small planets furnish the value

$$(5)\qquad \nu_4 = + 0''\cdot0012.$$

The mass of the Earth enters advantageously for its determination in three relations.

The first (A) of these relations * results from the latitudes of

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* In the original, a misprint 'rotations.'

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Venus at the moments of the transits in 1761 and 1769. It is independent of the uncertainty in the apparent diameter of the Sun, and possesses an extreme precision :—

$$\text{(A)} \qquad -0''{\cdot}53\,\nu + 24''{\cdot}6\,\nu_1 + 32''{\cdot}8\,\nu_2 - 1''{\cdot}86 = 0.$$

The second (B) is furnished by the discussion of the meridional observations of *Venus*, in an interval of one hundred and six years :—

$$\text{(B)} \qquad -0''{\cdot}76\,\nu + 24''{\cdot}6\,\nu_1 + 32''{\cdot}9\,\nu_2 - 2''{\cdot}00 = 0.$$

This agrees very nearly with the first (A), which is a proof of the accuracy, both of the observations themselves and of their discussion.

Lastly, we will deduce the third or (C) from the observations of the 1st of October, 1672. Richer at Cayenne, Picard near Beaufort, and Roemer at Paris, all compared *Mars* with the star ψ *Aquarii*, which was then occulted by the planet. Their separate comparisons differ respectively only by the very small quantities $0''{\cdot}5$, $0''{\cdot}8$, and $0''{\cdot}3$. On the other hand, Bradley has left us thirty-two meridional observations of the three stars ψ, ψ^1, and ψ^2 *Aquarii;* we have discussed these with great care, and here also there is every guarantee of accuracy :—

$$\text{(C)} \qquad 225''{\cdot}3\,\nu_2 + 29''{\cdot}5\,\nu_1 + 1398''\,\nu_4 - 21''{\cdot}86 = 0.$$

If, making use of the equations of condition (2) (3), (4) and (5), we eliminate the indeterminate quantities ν, ν_1, ν_2, ν_4, we derive from this process, by means of each of the equations of condition (A), (B), (C), treated separately, the following values for the ratio $1 + \nu_2$ of the new mass of the Earth to that which was adopted as the starting-point ; also for the value of the solar parallax deduced therefrom :

	$1+\nu_2$	Deduced Solar Parallax.
(A)	1,0917	$8''{\cdot}853$
(B)	1,0937	$8{\cdot}859$
(C)	1,0965	$8{\cdot}866$

It is well known that L. Foucault has found, by direct measurement of the velocity of light, adopting the constant of aberration determined by Struve, the number for π, $8''{\cdot}86$.

It appears from these results that the epoch has really now arrived when the value of the mass of the Earth, which ought to be introduced into the calculations of the *Mécanique Céleste*, can and should be derived directly from the motions of the planets, and no longer though the intervention of the solar parallax.

But it may be asked, does it follow that it is legitimate to conclude from the former by a reciprocal process the value of the solar parallax itself? Undoubtedly it would be, if we were certain that the celestial bodies which we take into account were

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the only ones that could come into consideration. But, whatever reasons we may at present have for regarding the aggregate mass of the minor planets as extremely small, it is nevertheless possible that their attraction, continuing to accumulate in the course of time, may at last become sensible; and it is proper to ask whether a proof of this can be found in the difference between the values of the parallax determined from the discussion of the movements of the celestial bodies, from the measurement of the velocity of light and of aberration, and lastly, from the observation of the transit of *Venus* over the Sun's disc. It is not easy to answer this question ; it depends upon the magnitude of the unknown influence, the determination of which is itself the matter under consideration.

If the discussion of the motions of the celestial bodies should give a value of the parallax larger by $\frac{1}{10}$th of a second than that which is deduced from the velocity of light, and than that which may be deduced from the transits of *Venus*, we might attribute this difference to the attraction of the minor planets, and so measure the amount of their influence. But if this influence is represented by a result corresponding only to $\frac{1}{100}$th of a second in the value of the solar parallax, all that could be concluded would be that the total mass of the minor planets is excessively small, but that its actual amount could not be measured.

Such may, in fact, be the case. The attraction of the minor planets must certainly, if sensible at all, be much more so in its effect upon the planet *Mars* than upon *Venus*. And since the discussion of the observation of *Venus* leads us to the same result, as that of the observations of *Mars*, and since also this result is the same as that derived by Foucault from the velocity of light, it appears very probable that the attraction of the minor planets amounts up to the present time to a quantity which may be neglected.

But it is necessary here to remark that as the angular errors introduced by the perturbations in the positions of the planets *Mercury*, *Venus*, the Earth, and *Mars*, go on constantly increasing, in the course of time an epoch must arrive when it will no longer be possible to put all these quantities in accord with each other, without perhaps introducing new forces. This is even now certainly the case with respect to regions situated between *Mercury* and the Sun. Thus there is a considerable quantity of matter which has hitherto escaped our regular investigations.

These things being so, it seems that Astronomy ought to enter upon a path somewhat new ; and I would venture to ask the Academy to interest itself in a number of pieces of work which have now become necessary.

(1.) In the first place, it is necessary to open what we will call, the better to express clearly our idea, *the taking account of celestial masses.** For this purpose, going back into the past, we should inquire with care the circumstances under which per-

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* In the original, 'Le compte des matières célestes.'

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turbing actions peculiar to such and such a planet have produced remarkable perturbations, and establish the equation of condition which results from this for the determination of its mass. In doing so, use ought to be made of all the past observations made at different observatories.

At the same time the most favourable future circumstances for the determination of masses ought to be investigated, and made known to astronomers beforehand, so as to secure obtaining the necessary observations.

Thus would result a union of conditions, the fruits of which would accumulate daily, and lead to the most important results.

(2.) We would ask physicists again to take up the *direct* measurement of the velocity of light. The Academy would without doubt obtain this from M. Fizeau.

(3.) The determination of the constant of aberration ought to be an object of attention to astronomers; it would be very interesting, at a time when the constant determined by M. Struve plays so special a part, to be possessed of the grounds of the opinion of that eminent astronomer on the degree of accuracy which he is sure of having attained.

(4.) Lastly, the determination of the solar parallax by means of the transits of *Venus* still retains all its interest, but conditionally on its being made with exceptional precision, so that the astronomer may be able to answer for it with an accuracy corresponding to $\frac{1}{100}$th of a second of arc, or about the $\frac{1}{900}$th part of the total value of the parallax.

With this limit of extreme precision the labour becomes one of the most delicate works of art, and one which ought only to be confided to men who have given special guarantees of their devotion to the enterprise, and who have themselves resolved to carry out the observations. We think the Academy should name these without delay, leaving to them the whole of the responsibility, but also providing them with every assistance in procuring the means of execution.

After the reading of this paper, some remarks were made by M. Fizeau and M. D'Abbadie on the question of the desirability of re-determining the velocity of light by the method of Foucault. M. D'Abbadie said that Foucault's result was, in fact, a happy coincidence. The base selected by that ingenious physicist was so small (only two or three metres, in fact, in length) that any error made in measuring it was enormously increased in the determination of the resulting velocity. But M. Fizeau had previously invented a much better method which rendered it possible, by selecting suitable stations, to take a base of more than 100 kilometres, which geodeters can measure within a decimeter. In making a new determination, M. D'Abbadie thought it extremely desirable to use this method in preference to the much less exact one of Foucault.